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1. The investigation of claims of telepathy, clairvoyance, veridical hallucinations and dreams, psychometry, precognition, dowsing, and other forms of supernormal cognition; of claims of supernormal physical phenomena, such as raps, telekinesis, materialization, levitation, fire-immunity, poltergeists; the study of automatic writing, trance speech, hypnotism, alterations of personality, and other subconscious processes: in short, all types of the phenomena called psychic, mediumistic, supernormal, parapsychological and metapsychic.

2. The collection, classification, study and publication of reports dealing with the above phenomena. Readers are asked to report incidents and cases. Names must be given, but on request will be treated as confidential.

3. The maintenance of a library on psychical research and related subjects. Contributions of books and periodical files will be welcomed.

4. Cooperating in the above tasks with qualified individuals and groups who will report their work to the Society.

MEMBERSHIP IN THE SOCIETY

Members, who receive the Proceedings and the Journal, pay an annual fee of $10. (One may become a Life Member or endow a Memorial Membership on payment of $200.) *Fellows,* who receive all publications of the Society and are afforded special library privileges pay an annual fee of $25. (Life Fellowship, $500.) *Patrons and Founders*: For those who wish to make a still larger contribution to the Society's work, these classes are open at $1000 and $5000, respectively.

It is to be remembered that membership in a scientific society means more than merely a subscription to its publications. The work must be carried on largely through the income from membership fees. Therefore members, old and new, are urged to make their membership class as high as they feel they can. *If a comparatively small proportion of the present members went one class higher, the money available for research would be more than doubled.*

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Fiftieth Anniversary Celebration

The American Society for Psychical Research will celebrate its Fiftieth Anniversary on Friday Evening, March 2nd, at the Women's National Republican Club, 3 West 51st Street. Members who wish to dine at the Club before the meeting will kindly make their reservations with the secretary at 880 Fifth Avenue. The program of addresses by leading psychical researchers will begin at 8:15 P.M.

The meeting will commemorate the founding of the Society by Professor James Hervey Hyslop (1854-1920), following the death in 1905 of Dr. Richard Hodgson who had served as secretary of the American Branch of the English Society which was dissolved shortly thereafter.

Professor of Logic and Ethics at Columbia University, Professor Hyslop resigned his post in 1902. For over a decade he had recognized psychical research as a subject of great and growing importance. As director of the newly-founded American Society he brought the technical methods of the scientist to the investigation of psychic phenomena. He personally selected and edited all the material that appeared in the vast volumes of *Proceedings* and JOURNAL and published many books and articles in magazines for the ordinary reader. He delivered lectures on psychical research throughout the country, attended to all financial matters, secured about $185,000 of endowment as a nucleus for a permanent Foundation—all this wholly without compensation. He provided for a competent successor to

carry on his work by engaging Dr. Walter Franklin Prince as his assistant in 1917.

It was after prolonged and arduous experiment that Professor Hyslop discarded his erstwhile advocacy of telepathy as an explanation of the results he obtained, and he became convinced that survival of personality after death was the more reasonable interpretation. After he reached this tentative conclusion, he adopted it as a working hypothesis.

Apart from his abilites as an investigator Professor Hyslop had a genius for organization. When the first JOURNAL of the Society appeared in January, 1907, his list of members comprised 300 names from 32 states in this country. This was a single-handed achievement. Before his death the list was quadrupled.

Now that the Society has reached the half-century mark it is well to pause and pay tribute to its Founder. The organization he built so wisely has withstood the vicissitudes of time and cicumstances and assured the continuity of the Society to which he devoted the best years of his life.

Notice of Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc.

The Annual Meeting of the Voting Members of the American Society for Psychical Research, Inc., will be held at the office of the Society, 880 Fifth Avenue, Suite 1A, Borough of Manhattan, City of New York, on Tuesday, January 31, 1956, at 4:00 o'clock in the afternoon, for the election of five Trustees and for the transaction of such other business as may properly come before the meeting.

Lydia W. Allison, *Secretary*

Psi and the Problem of the Disconnections in Science [1]

JULE EISENBUD, M.D.

I

Parapsychology deals with correspondences between events. These correspondences are occult in the sense that whatever is responsible for them is, in line with a standard dictionary definition of the term occult, "not immediately known; perceivable only by investigation."

Up to recently it was customary to designate such correspondences as telepathic, clairvoyant, precognitive, psychokinetic, or by roughly equivalent terms. In the past few years, however, the term "psi" has come into increasingly widespread use in relation to the class of occult correspondences formerly encompassed by such terms as those given above. Most investigators have come to feel that there are no compelling grounds for making any assumption other than that behind the various phenomenological forms and manifestations in the realm of occult correspondences there is one underlying process. This fundamental what-is-it, which they presume to be the common noumenon behind the phenomena of telepathy, clairvoyance, psychokinesis, etc., they agree to call psi.

It is felt that the term psi succeeds in eliminating some of the possibly misleading implications with which the older terms were burdened. In designations like "a psi event," "a psi relationship," "psi phenomena," it is supposed to be understood that psi refers to nothing more than the still unknown means or process by which a given event, relationship, or phenomenon is brought about. It is often more or less automatically assumed, however, that this means or process is an actual something-or-other which in principle can be positively identified (that is, not solely by exclusion) and differentiated from a set of non-psi processes. The term psi, in other words, is not limited to the designation merely of a *class* of events which lends itself to differentiation solely on certain phenomenological grounds from other classes of events; it is frequently used to refer to a quite special and even, as far as all the natural forces and processes now known to science go, uniquely operating principle which is postulated to be the particular thing-in-itself that, working from below, as it were, is inherently responsible for the fact that this class of events can be more or less sharply differentiated from other classes of events.

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[1] A condensation and abridgement of a paper presented at the First International Conference of Parapsychological Studies, University of Utrecht, Holland, July 30 — August 5, 1953.

Despite the lack of clarity surrounding the concept of psi and the looseness with which the term is often used, in the minds of some parapsychologists it has come to be looked upon as a force of some kind — a "spiritual force," a "psychic force," a force vaguely associated with thought — at all events, a "non-material force" insofar as it is generally held to be "independent of time and space." Somehow, the notion of a *psi force* has taken deep root, and it is this "force" that, in one form or another, is held to provide the causal connection between the events that make up those correspondences that would otherwise be completely inexplicable. (One need not cite the parapsychological literature at length to show that this is a widespread notion about "psi," whether or not explicitly stated as such.)

It would be highly desirable to be able to arrive at a more precise conception of what we mean by "psi." Although we do not know what inertia is, or momentum, or electronic charge, we nevertheless have a high degree of clarity about what we mean to signify when we use these terms. The meaning of terms like these, and of many others of similar stock, is derived directly or indirectly from a group of congruent pointer readings which vary consistently in relation to each other under circumstances which we are able to describe with great exactness. From this we are able to infer the highly consistent — in fact predictable — quantitative variation of *that which we agree to call inertia* (or momentum or charge) under varying conditions that we are able to define and specify in terms of other pointer readings with a high degree of precision. The underlying principle, the "thing-in-itself" behind the dynamical concept, we have no way of getting at; but so long as we have a consistent means of mathematically relating events in a given domain, we are content to speak of a "force." The notion of force has no other meaning. It is a metaphysical skin drawn over a skeletonized set of mathematically stateable relationships.

It is the absence of relationships of this kind, however, that characterizes the correspondences dealt with by parapsychology. It is this lack — and mostly this lack, moreover — which seems to set these correspondences apart from most other correspondences known to science. If not for this lack, perhaps, there would be nothing that would be considered occult, nothing "paranormal" about any of the phenomena we now classify under psi. What measurements we do make have to do with chance (telling us merely that we have correspondences on our hands, nothing about what is responsible for them) and latterly with personality characteristics. But neither of these classes of measurement is of the type of those tightly interlocking pointer readings which provide us with the most familiar basis on which we conventionally define in a positive way a so-called causal

connection between events.[2] As a matter of fact, all we have according to this way of structuring events are what might be termed causal *dis*connections. These disconnections in our process map of science — our simple inability to account for any of our observed correspondences by means of "force" assumptions that are in any way explanatorily continuous with those used to link other correspondences known to science — are all that the term "psi" can legitimately be construed to refer to.

Perhaps the gap that confronts us represents nothing more than some inadequacy in the grammar of the language with which we describe nature, a developmental anomaly resulting in a baffling lack of congruence between our descriptions of various aspects of the whole, or at least what we have always been tempted to assume to be, despite our clear difficulties in picturing it as such, a well hung-together and smoothly running whole. But however this be, we cannot make a disconnection into that type of connection we call a "force" by simply giving it a name and imputing to it a negative qualification — something that tells us how "it" does ***not*** act — like "psychic," "spiritual," or "non-material." Such terms still fail to state anything more than that we are confronted by a contentless hole (descriptionwise) in our process map of nature, an abrupt and disconcerting chasm separating our techniques of describing certain correspondences from our techniques of describing other correspondences.

Have we then nothing more in "psi" than a new name for something essentially indistinguishable from the demons of old, which too were "force" assumptions invented to fill gaps in the presumed causal fabric of things but which lacked the positive identifying credentials which alone are capable of being harnessed for the work of scientific inquiry? Is "psi" nothing more than a modern-dress latecomer to the demonological hierarchy?

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[2] The closest we have come in appearance to such measurements are those reported by Crawford. (*Experiments in Psychical Science*, New York, Dutton, 1919; *The Psychic Structures at the Goligher Circle*, Dutton, 1921). A fundamental methodological difficulty in this work, however, is that the measurements reported by Crawford were not confirmed by a sufficient number of independent investigators to minimize, if not to obviate, the possibility that the medium was simply producing data to order. Crawford was an engineer who thought entirely in terms of mechanical "forces," beams, cantilevers, and the like. It is entirely conceivable that the medium, who after all was demonstrably capable of a wide range of physical manifestations, was unconsciously serving as an accomplice "before the fact" by acquiescently dishing up data in a form that Crawford, an engineer, would find most comprehensible and satisfying.

This methodological difficulty is inherent in all our experimental investigations of psi. Once events of the psi class are assumed, moreover, it becomes necessary to redefine the basic concept of measurement as it applies in all science.

II

Before attempting to deal with this question, let us first look at types of disconnections met elsewhere in our process map of nature. As it happens, such breaks in our map are not rare, and we do not have to look far to find other areas of experience where we have never really succeeded in getting behind a description of events in terms of correspondences that remain — for all our efforts to account for them coming into existence *as* correspondences — occult. These correspondences do often enough group themselves into certain regularities and can to this extent be described with more or less satisfying consistency; but they nonetheless remain correspondences of unknown origin, correspondences with, so to speak, no visible or, so far as our conventional means of structuring experience go, invisible means of support.

The most familiar example, and the one ostensibly in closest connection with our present problem, is the broad group of psychophysical correspondences that has always been a central preoccupation of philosophy under the heading of "the relation of mind to matter." The disconnection here has been dealt with mainly in two ways: One, the postulation of a connecting "force" principle which has been given various names (the soul, for example) and which is essentially demonological; two, the attempt to restate the problem in such a way that what appears to be a duality can be reduced to unity. The latter method of attempting a resolution of the issue might be quite satisfactory if the "body-mind" problem existed alone, isolated from the other data and problems of science. But it does not; in fact the more data we gather from other fields, the more this problem, in one form or another, intrudes into everything. Of this, more later.

Another area (or possibly the same projected onto different coordinates) in which we are confronted by a major theoretical disconnection is that of the relation to each other and to the whole of all the events in nature to which the theory of probability is applied. There are currently several differing approaches to probability theory, but the objective of each is to arrive at statements which describe the ways in which events which are axiomatically held to be genetically independent fall out in relation to each other. Herein lies the paradox. One starts with events that are assumed to occur completely independently of each other and ends up with events that, insofar as they fall out in the aggregate in over-all conformity to definite patterns of expectation, seem to show a mutual dependency of some sort that has developed en route, a lawfulness that has crept into the situation somewhere between the initial state-

ment of the rules of the game and the final scoring of results. At any rate, while it may be possible to *describe* with the consistency demanded by logic what takes place when a hundred or a thousand events, each independent of every other at the start, somehow whip themselves into a regimented outcome, it is not possible to say how this comes about. For all our devices of logic, there remains a descriptionless gap between one toss of a coin and any other (to take the most familiar case).

No less enigmatic than the theory of probability upon which it rests are those aspects of quantum theory which provide descriptions of events on the microphysical plane. Exactly the same difficulty is encountered here since our only method of describing what occurs in this realm is to utilize the language of the mathematics of probability. Just how it happens that ruggedly individualistic waves or particles, each conceived of as a law unto itself, somewhere along the line decide to waive their potentially anarchical individual rights and prerogatives and swing good-naturedly and with remarkable *esprit de corps* "into accordance with" the laws of probability, thus transforming themselves into highly respectable and well-ordered macrophysical events — this we cannot account for. We are left again with nothing more than simple, unexplainable correspondences between events that cannot be related except in terms of probabilistic correspondences which themselves have in some inexplicable fashion "jes growed," like Topsy. We have the spectacle of one disreputable character vouching for another.

The one other major segment of nature where we have had to put up with a kind of disconnection which has never been successfully resolved is that in which the law of gravitation is held to apply. Here we have a set of correspondences capable of mathematical definition but with no assumptions as to the causal or dynamical connections involved. "Force" turns out to be an unnecessary concept in the geometry of mass and field, just as it is in the theory of probability, and the technique of description used outflanks the "action-at-a-distance" difficulty by ignoring it.

III

We see thus that, as far as our means of describing what goes on in "causal" terms go, the gaps between our correspondences in the thought-action sphere, between any two events in a probability series, between two definable states of an electron, and between any two bodies in a gravitational system, are just as wide, just as empty of the "stuff of causality" and, in this sense, just as occult, as any two events that go to make up a correspondence of the psi class.

The question now arises: Do we have any particular justification for treating events of the psi class in isolation from the events which make up the occult correspondences in other spheres, and in postulating a special principle applicable primarily to these "psi" correspondences and without reference to the disconnections encountered in our descriptions of other segments of nature?

This question can of course also be posed the other way around; but let us simply recognize the fact that nobody but parasychologists would ask it this way, since the data of parapsychology have no official standing in science. Before going further, however, let us take a preliminary look around to see what other people are doing about *their* disconnections.

The first thing we notice is that in regard to what assumptions it might be convenient to make about this or that disconnection, no uniform convention has been agreed upon by scientists of diverse fields or even by scientists working in the same field. The positions adopted in regard to the treatment of disconnections seem to be a matter of individual preference, and the three main categories into which these positions can be divided appear to be correlated along vocational lines in as little degree as political attitudes.

One convention which has been in high favor among quantum physicists, relativity physicists, and biologists is to treat the problem as one without immediate relevance to the tasks in hand. For working purposes, anything on the order of a "causal" bridge over the disconnections met with is considered to be an unnecessary assumption. In each area the technique of description is held to be adequate to the events dealt with to the extent of providing the necessary framework for the continuance of scientific operations. So long as the structural formulae for relating observed events to each other and for the prediction of as yet unobserved events seem to work, nothing further is demanded. The question of whether or not to impute a causal character to whatever reality may exist beyond the observable data themselves is held to be not a scientific question but a metaphysical one; and the fact that scientists nevertheless spend their time endlessly debating this question is held to be not germane to what they are supposed to be doing *as* scientists.

A somewhat different position, which spread rapidly from the quantum physicists to scientists in all areas after Heisenberg enunciated in 1927 his famous indeterminacy principle, is to deny not only the operational necessity of the causality principle but the very existence of any such principle in nature itself. To the indeterminists, what has looked like causality is at best an illusion, a statistical artefact. There is no nature, no reality, say they, apart

from what we observe; and since we can observe only by means of metrical devices and techniques of description which embody fundamental discontinuities in the mathematical sense and disconnections in the dynamical sense, the grounds for belief in a causal "beyond" in which any such nonsense as "ultimate" process in nature may be imagined to take place simply disappear. The only way we can define nature is in *a*causal terms; and that, they state with perhaps the maximum degree of certainty they can muster, is that!

An inability to accept either of these first two positions has led a growing number of scientists to a third view which, as it turns out, is simply a revival, with very few modifications, of one of the most ancient and widespread attitudes toward the problem of causality in the history of mankind. But first as to the grounds on which the first two positions are sometimes found wanting.

That there can be a sharp distinction between science and metaphysics is disputed. Science, it is claimed, cannot even begin to formulate physical theories — or theories dealing with any type of event, for that matter — without making the most sweeping metaphysical assumptions and without taking into account unobservables and unmeasurables of all kinds. What is more, the distinction between what is observable and what must be assumed is becoming so increasingly blurred that to imagine that we are in a position clearly to say what are "necessary" working assumptions and what are not is a hazardous presumption.

There are more fundamental grounds, however, on which the utter abandonment of something like causality, whether as a working principle or as an ontological one, is unacceptable to many. Such a theoretical position, it may be pointed out, involves a peculiar contradiction in the entire structure of science and scientific method. These have but one consistently tenable objective, an objective to which all part-operations are necessarily subordinate: The construction of hypotheses which replace chance with definable nonchance factors in the description of the universe. The establishment and the carrying out of this program on a level of conscious awareness is *the* feature which distinguishes scientific activity from all other activities of man. To have all our operations, however, culminate in an hypothesis which accords chance a fundamental and sovereign role in all events is just as absurd (for science) as to set up a government at whose apex sits a supreme court which decides every issue in jurisprudence on the assumption that all government is false and by right does not exist. It may be claimed, moreover, that what the acausalists and indeterminists *actually* do is accept government *de facto* while denying it *de jure*, which puts them in a

position hardly less anomalous than that of the woman who, when asked why she didn't finally marry the man who was the father of her ten growing children, replied, "He just doesn't appeal to me."

Now when men of science begin looking for a causal principle they can't quite put their fingers on, which they can't define in terms of an unambiguous set of symbolic relationships, they not infrequently end up with the idea of God, or at least some transcendental principle which amounts to the same thing. This is, of course, exactly what every major metaphysician from Aristotle through Whitehead has held to be the inevitable logical consequence of the global approach in which things and events are considered all together. While today a high degree of sophistication seems to be required before this idea takes hold, this was not always the case. The men of science of Newton's era took it for granted that nature had a Supreme Author, and that their efforts were of course merely attempts to piece together His Work. The mystery and awe they felt were not the blank confusion of today, since there was no mystery in principle: No disconnections existed in the Mind of God.

At any rate, this is a third way of attempting to deal with the problem, an increasingly popular way among men in the top ranks of science and mathematics who are becoming less and less diffident in expressing their conclusions. The disconnections are recognized, held to be a legitimate concern, and dealt with by imputing all occult causal bridges — both in their logical and dynamical aspects — to a Divine Principle or the Mind of God (in the metaphysical sense). These people may agree that the God concept may be as unnecessary for the adequate performance of the daily nine-to-five tasks of the scientific investigator as is the concept of the workings of a huge corporate structure to the petty bookkeeper in one of the branch offices at the periphery of the system. On the other hand they find it difficult to account for the peripheral transactions except in terms of the existence of the parent corporation.

IV

There are then these three major positions on the problem of disconnections taken by scientists today. And since there is no uniformity in the treatment of this problem, it should, needless to say, be permissible in all fairness for parapsychologists to adopt any one of the three positions, which we might characterize as the "No need to worry about it," the "There *is* nothing to worry about," and the "Let George do it" positions.

The matter could conceivably rest this way indefinitely were it not for the fact that even the small measure of tolerance shown among

scientists by the adherents of one doctrine toward the heresies of the other groups does not extend toward the parapsychologists when the latter attempt to adopt one of these positions in relation to their own problem.

It is just as feasible, for instance, to adopt the first position — that for working purposes one can do without metaphysical luxuries — in the case of correspondences of the psi class as it is in the case of the occult correspondences in the other areas cited. Up to this point, in fact, this has been effectually the state of affairs. It has been possible (1) to classify data, (2) to devise experiments, and (3) to make a promising beginning toward the integration of psi data with the major hypotheses of currently acceptable psychological theory — all solely in terms of correspondences of unknown origin established according to the conventional rules for the making of correspondences. The hypothesis of a psi "force," as the form in which the assumption of causality is put, is virtually impertinent to the actual tasks of investigators in these areas; and the question of a causal bridge over the disconnection has had, whether or not acknowledged as such, the essential status of a metaphysical issue.

But the license to ignore what they consider to be metaphysical questions which many statisticians, physicists, and biologists allow themselves they do not grant to the parapsychologists. Physicists may state: "Our only way out seems to be to take for granted the fact that space has the physical property of transmitting electromagnetic waves, and not to bother too much about the meaning of this statement"(3); but they are not at all willing to say to the parapsychologists, "You just go ahead and make what order you can of the events you feel justified in arranging as correspondences, and don't worry too much about how it is possible for these correspondences to occur at all." Scientists in divers fields can and do shrug off *their* unexplainables in this way, but the no whit more "miraculous" data of the parapsychologists are considered by them to be outside the province of science precisely because they are unexplainable in terms of conventional causal assumptions. It would appear that among scientists the absence of sauce for the goose is one thing while the absence of sauce for the gander is quite another.

It is also possible — or at least permissible on comparative grounds — to espouse a more thoroughgoing doctrine of acausality in relation to events of the psi class. In this case too, the adoption of such a credo, or rather *non-credo*, would make little difference as far as the present program of classification and research in parapsychology goes. It is plain that if we are so disposed as to be able to conceive of what is termed the fundamentally statistical

character of events, then psi data, like physical data, need nothing further to justify them than the probability determinations they now have and in terms of which they are in fact construed as significant data.

Here again, however, it has become plain that the ranks of scientists close into a solid wall of opposition to any such point of view. An attempt on the part of parapsychologists to rest squarely and exclusively on their statistical determinations (it makes no difference in such a case whether an outright doctrine of acausality is explicitly asserted or not) seems to call forth a protest of impossibility which stems plainly from the implicit attitude that statistics are not enough in the absence of a justifying causal concept. This, as is well known, has led to a type of attack on the experimental and statistical procedures employed which has not been similarly the case in an effort to batter down the causeless and in this sense equally "impossible" data of the physicists. Even the most determined anti-determinists seem to resist the logical extension of their doctrine (or dogma) to the data of psi, which is as absurd as one atheist claiming that *his* atheism is a higher atheism, one more worthy of faith and belief, than the insubstantial and positively ungrounded atheism of his neighbor.

Historically the most popular tradition in parapsychology, from the days when it was all psychical research, is that which corresponds to the third, or "let George do it" tradition. George has been variously conceived of and has had different *noms d'action* under one or another spiritistic hypothesis, has even achieved the status of Deity, and has now become quasi-mechanized under the alias of "psi"; but George is still the same old George for all that — a creation, a moving principle, a "force" with plenty of *raison d'être* and yet with no more positively identifiable existence than the illustrious Putois of Anatole France's allegorical extravaganza of that name. Putois, whom nobody had ever seen, became endowed with a reality in the minds of the simple villagers who created him as indubitable as that of their own selves, simply because he came to be the perfect and natural explanation of a whole series of otherwise inexplicable events, such as the pregnancy of one of the town's most unassailable virgins. The fact that there was not a shred of first-rate evidence that any one had ever seen Putois, talked to him, heard him, felt him, smelled him, or received a bona fide kick in the seat by him, did not render him any the less credible as the actual agency of a great many otherwise disconnected happenings.

It is questionable whether there is much of a difference between psi, conceived of not solely as a category of phenomenological classi-

fication, but as some sort of dynamical "force," and the transcendent principle which metaphysicians and latter-day scientists identify generically as God. Perhaps the main point of distinction is the former's provincial character, like one of the old tribal gods who later ceded to Jahveh. Psi does things only for the parapsychologist, while the God of the metaphysician has a blanket commitment to do things for all logicians and natural scientists, to fill up all blanks, gaps, holes, discontinuities, and disconnections wherever they may be. In any case, it is precisely the unlimited flexibility of such a concept that has bothered the practical scientist who likes to be weighing and measuring and who prefers a specifiable datum in hand (even if the datum be his own hand) to a deity in the bush (even a burning bush). And it is highly doubtful, furthermore, whether the very scientist who today falls back upon the God concept when he comes to the end of his technical and theoretical tether would grant the same sanctuary to the equally desperate parasychologist.

At all events, it appears that all of science is in the same boat as far as its fundamental assumptions about the interconnections between events go, with parapsychology neither more nor less adrift than other disciplines. Most of these deal with correspondences between events which cannot be accounted for except in statistical terms; but the theory of probability behind this system of accounting turns out to be based upon a set of correspondences no less obscure than those they are used to "explain." It is like finding that all the alleged gold in Fort Knox is nothing but paper money after all, and that all our transactions have been carried out without real collateral.

V

Several scientific authors have recently suggested compromise solutions to the problem of the disconnections within their special fields of investigation but have failed to recognize that the problem they are dealing with cannot be given a narrowly limited treatment. Schroedinger and Lillie, speaking for biophysics and biology respectively (14, 10), have tended to make an implicit distinction between living and non-living systems, and have given the impression that the central problem, as they see it, is simply that of accounting for the directive factor which opposes and alters, as it were, the general laws of probability in the special instance of life. That the problem, however, is exactly the same in non-living systems, where even what we call randomicity stands in need of some sort of explanation, has been brought out by Margenau in his inquiry into what makes quantum-mechanical systems work (12).

The one who most clearly saw that it made no difference whether we were dealing with living or non-living systems, with anti-chance or chance, and who tried to formulate the problem squarely in terms of probability theory itself, was the philosopher-mathematician Karl Marbe (11). This author saw in the paradox of the fact that probability had laws at all and that it worked, what many metaphysicians had already grasped in terms of ontological and cosmological necessity — that nothing in the universe would work, neither atoms nor animals nor astral systems, in the absence of what amounts to a communicating dependency of each event upon every other event and upon the whole, a state of affairs that more or less implies (if we are to avoid unconditional surrender to the "Mind of God" hypothesis) cognitive, reactive and "volitional" aspects of every element in nature, whether as single atoms or as complex polyatomic systems, living or non-living. In his exhaustive study of "what makes probability run," Marbe was led to deny the statistical independence of successive trials in a coin-toss series and came to the same conclusion in regard to every other type of statistical series: i.e., that in a sense nature *does* have a memory (at least he suspected that the answer to the riddle lay somewhere in the psychological sphere, just where he could not finally say). Had Marbe given some thought to the implications of the psychical research of his day, he might have come somewhat closer to unravelling the puzzle which he saw so clearly.

VI

As far as I know, the first major attempt to view the data of psi in connection with the data of both physics and psychology was that of the physicist Jordan (8). Drawing a parallel between the data of these disciplines on an epistemological basis, Jordan attempts to resolve all contradictions between the three sets of data in terms of Bohr's principle of complementarity and the concept of the unconscious, as described by Freud in connection with the phenomenon of repression and as extended by Jung into the idea of the collective unconscious. Jordan sees no causal connection between these sets of data but holds one to be complementary to another in the sense that what is actually done by the observer in the act of measurement determines and at the same time limits the type of datum that results. He extends the psychological concept of repression to the notion that one set of data "represses" another. In terms of the logical relations involved, he feels, this can be asserted just as validly in respect to the position and velocity of the electron as it can in relation to the two phases of a dual personality, only one of which is objectifiably in evidence at a given time. He feels that in some way "the Uncon-

scious" is the missing link. At any rate, paranormal phenomena he holds to occur in the "collective unconscious," which stands in complementary relation to the "collective conscious" space in which physical phenomena (e.g., those linked by radiations of one sort or another) take place. Although what happens in these two "spaces" may be said to be in complementary relationship, there is no causal relationship between the two. This, states Jordan, follows from the fact that physics can assert with complete certainty that there are no undiscovered radiations which can now or ever be brought forth to explain what happens in the parapsychical realm.

A theory somewhat similar to that of Jordan has recently been brought out by Jung (9). This author too points out that the conventional notion of causality is inapplicable to psi phenomena insofar as these are independent of space, time, and energy transfer. He proposes as a hypothetical "explanation factor" in the case of psi phenomena a kind of relationship which he terms *synchronicity*. Synchronicity may be said to obtain where events are linked together in a meaningful but in nowise causal manner, the meaningfulness of a relationship being defined in terms of archetypal ideas existing in the collective unconscious. In other words, archetypal ideas, springing from the collective unconscious, manifest themselves as psi occurrences. Jung too falls back on what amounts to a principle of complementarity in holding the two types of order in the universe — the familiar causal order and the order of events linked by synchronicity — to be mutually irreducible.

Both Jordan and Jung do indeed succeed in showing that the disconnections in data of the psi class are in principle no less embarrassing in the total picture of science than the disconnections in any other class of data; but at the end they leave us with a jigsaw puzzle which somehow fails to fit together. Jordan stresses the epistemological limitations which make it impossible to assert that parapsychical phenomena *cannot* take place, and Jung merely postulates a new principle, a sort of paracausality, by way of justifying the existence of these phenomena. But at this point both leave the world in pieces, divided into the things that are Caesar's and the things that are God's, and with little concern over the fact that a big problem remains so long as Caesar and God are left still not communicating with each other but either "repressing" one another or working side by side, amicably enough and on the whole not getting in each other's way, but in the last analysis quite independently.

The problem, however, is not simply that of staking out the relative domains of differing types of correspondences in a universe big enough and variegated enough to include them all, but of accounting

for the fact that there exist correspondences to be accounted for in the first place. This now brings us back to the question posed earlier: Are we justified in treating all the occult correspondences with which we have to deal, including those we designate as of the psi class, in isolation from one another?

VII

It appears that this question can be argued from many points of view. In many quarters, moreover, it is conceded to be unlikely that a "solution" of the problem of the whole will eventuate solely from the methods of science. Many feel that there will always be something left over, something which cannot be assimilated or even accounted for by any system, however self-consistent in itself, which is forced to abstract for the sake of logical comprehension that which is, if it is anything at all, ineffable, unnameable, and perhaps even unthinkable in the ordinary sense. This is the difficult-to-express insight which the great mystic philosophies have all tried to convey.

What we might strive to achieve, however, is at least a degree of consistency in our ordering of experience that does more than simply admit correspondences of the psi class into a crazy quilt structure on the assumption that one more unassimilable patch isn't going to make much difference. It may be possible to show that it *does* make a difference, and that the difference is in the direction of an increased ability to describe and order experience as at least a fictional continuum. To my mind a proposition which has been asserted so often that it has almost become the watchword of parapsychology — that "paranormal phenomena will not fit into a niche in the existing scheme of science" (Tyrrell) — does not possess the validity that appears to many so self-evident. It can be argued, on the contrary, that paranormal phenomena *do* fit into such a niche, that the niche in fact calls for something like paranormal phenomena to fill it.

For the following argument, a few ground rules must be laid down and clarified for at least minimal working purposes. Actually these are all highly controversial and admit of almost endless debate, but for our purposes let us try provisionally to accept them as axiomatic.

All we ever do in science is arrange and order sets of correspondences in ways that result in the maximum of meaning to us. *We* establish the rules for the making of correspondences, and *we* are the ultimate arbiters of meaning. We have worked out certain techniques (for both these aspects of our behavior) which have at least the capacity for facilitating maximum agreement *among ourselves* on the question of what the rules are to be and what we mean

by "meaning." In this wise we have arrived at working fictions which we term lawfulness, chance, causality, force, and other abstractions.

When we make (or, as we customarily term it, "observe") a correspondence, we are utterly without means for ordering it in relation to other correspondences we have made except in terms of still other correspondences we have made. This system of kiting correspondences is all we have, and is as close as we ever get to the bottom of notions like "force" and "causality," and to explaining what takes place in a solar system, a mechanical system, a kidney, or a group of people acting in a specifiable way. In none of these areas can we do better than "explain" (i.e., order into what *we* consider to be a meaningful arrangement) one set of correspondences in terms of another. One set constitutes, so to speak, the warp of our fabric, another the woof. The warp and the woof hold each other in place, order each other, as it were, but do not constitute, except in the sense of the patterned arrangement that results, a continuum. In logic we can make any number of ordered arrangements which satisfy in this way our criteria of meaning. In pragmatic science we are to a great extent limited by a set of firmly established correspondence-making habits which are altered only slowly in the development of new habits.

Now events which we have come to designate as of the psi class constitute a set of correspondences which we have so far had little success in meaningfully ordering in terms of those correspondences which we abstract into the familiar notions of space and time. As a matter of fact we have much greater success in establishing correspondences of the psi class if we abandon completely any attempt to relate events according to time and space measurements. We can make an analogy here to the superfluity of spatial measurements in many other correspondence systems — for instance that of probability itself.

When we adhere to our program of abandoning completely any reference to spatial and temporal coordinates (i.e., correspondences) and view certain events against a backdrop of a system of correspondences developing from the attempt to order the data of human behavior according to the correspondences that result when we relate the self-observed behavior of the individual to his non-self-observed behavior (i.e., according to the so-called psychodynamic principles based on the concept of "unconscious mental functioning") we find a very rich field for the making and ordering of correspondences which, according to our rules, we may reasonably presume and

designate to be of the psi class.[3] This system of making correspondences appears provisionally to justify the following inferences, according to the published work of several psychoanalytic investigators (1, 2, 4, 6, 7, 13, 15) and the still largely unpublished studies of my own: (1) a large and variegated (i.e., in respect to phenomenology and conditions of appearance) segment of events can now be brought into the class of psi correspondences which otherwise would pass unremarked and unconnected; (2) a much larger than heretofore believed probable segment of the general population (in fact, by not unreasonable presumption, the entire population) lends itself to a certain ordering of behavior in terms of psi correspondences; (3) a reasonable model for the interaction of the members of psychologically defined groups can be constructed along the lines of psi correspondences to supplement but also to integrate descriptively with models of so-called "interpersonal relationships" developed along the lines of other systems of correspondences.

VIII

What can be construed from the study of psi correspondences made from the psychoanalytic observation of small groups of individuals is that people appear to be in effective, albeit "unconscious," communication through other than so-called normal means and in ways in which they mutually influence each other's behavior (thoughts, feelings, actions). This is not continuously observable in the case of any individual or group, and thus we cannot strictly determine from our data whether this "psi-conditioning" of behavior (for lack of a better term) *occurs* only under certain conditions or can be *observed* only under still but poorly defined conditions in the observer as well as the observed. Suffice it to say, however, that many of our data show this type of psi-conditioning of behavior under conditions so similar to what appears to obtain in the most prosaic and humdrum circumstances of life — so far from what are usually considered catastrophic or stressful circumstances — as to invite more than the suspicion that this sort of subliminal determination of behavior may indeed be a general feature of all interpersonal relationships. For reasons which are gradually becoming clear, this presumed state of affairs has to be denied by

[3] Here, however, is precisely where some parapsychologists seem to balk. Despite a good deal of abstract talk about the need to apply the psi hypothesis along broad lines, the moment this is attempted in this or that concrete instance in an effort to achieve a meaningful ordering of otherwise incomprehensible human behavior, an outraged cry arises as if the psi hypothesis were being rudely wrenched from its secure moorings in a glass case and being made off with. People who insist on their money earning interest should not complain that they cannot also have it as gold in their stockings.

what we conventionally regard as the conscious part of the self. This part of us has to adhere to the fiction that we are perforce constrained through natural limitations to live for the most part cut off from, and unreactive to, what goes on about us in a chance-ridden universe, and that the avenues of determination for our behavior at any given moment constitute at best a comparatively thin network in the vast oceans of freedom around us. This view of things is given great support by our seemingly fresh and "unpremeditated" reactions to events which break upon our awareness in the so-called normal manner, and our seeming inability to avert "unforeseen" dangers or to achieve professed goals successfully and without misadventure. But psychoanalytic studies of psi-conditioned behavior tend to show that these are often "cover reactions" at least partly designed to support the fiction adopted by the conscious part of the self. This fiction, at any rate, is easily enough rationalized, as only rarely do psi correspondences which require no particular psychoanalytic decoding come spontaneously to light, and when these do, as every parapsychologist knows, everything in the conscious ego (so to speak) of civilized society seems to conspire to discredit their authenticity, their significance and their power seriously to affect the prevailing belief in the essential separateness of individuals. Some light on the origin and nature of this resistance can be shed by psychoanalytic and anthropological studies on an aspect of psi that appears to be decidedly unwelcome even to the parapsychologist — namely, its active and destructive potential.

One must remind oneself of course that such a schema as has been suggested here of the kind of patternization that can be achieved in interpersonal relationships by means of a psi-correspondence assumption is ideal — ideal in the sense that one cannot actually pick out at random a segment of historical reality and plot it out completely, and with no lacunae left to account for, in these terms. But this is true in one degree or another of any assumption we might entertain and apply.

What is perhaps of more relevance at the moment is this: If we do make the generalized assumption that the human macrocosm can be descriptively ordered along these lines, and if we stop worrying for the moment about what the psi correspondence "really" is or represents, is this in any way helpful to us in ordering other segments of experience? True we have, one way considered, a basic disconnection on our hands. Can we, however, manage to make do with something on the order of this one basic disconnection in other areas of our scientific picture map, or need we continue to have many disconnections, with disconnections between these disconnections? Can we in some way turn several into one?

IX

These questions can at present be only tentatively explored, and any suggestions that can be made are perhaps far yet from the stage where they can be rigorously formulated for decisive testing. But since we are at the moment primarily interested in a general descriptive framework in terms of which we might be able to order the data of several areas of scientific correspondence-making with greater consistency than obtains at present, we might do worse than attempt to apply toward this end a provisional model developed from the study of psi-conditioned human interrelationships. For this, several provisional assumptions would seem to be required, of which the two major ones appear to be as follows: (1) It is possible to order any given set of meaningful correspondences in terms which satisfy certain criteria for meaning in the ordering of any subset, and vice versa. Here we shall have to pass over the difficulties involved in the definition of sets, subsets, criteria for meaning, etc. We shall simply take it that all our present scientific data can be ordered as a set of correspondences, and that the data we divide into physical, biological, parapsychological, etc., can be ordered as homologous subsets. (2) The correspondence we now designate as that of the psi class can be taken as the prototype of every correspondence we are able to and in fact do make in science.

If now we take as a model of all event systems (i.e., all correspondence systems) that segment of the macrocosm which includes the observer and correspondence-maker himself, we are able to discern some method in the madness of the elements of any set. What we are attempting by taking the model of psi-conditioned human interrelationships as a model for all relationships which can be statistically defined is to introduce the notion of a dynamical system geared not to the categorically indeterminate vagaries but to the intercommunication-based responses and "strivings" of the individual element in such relationships. We can, in short, arrive at a means of describing the ways in which the laws of probability are carried out.

This is a crucial advance. We are perhaps as far from understanding the ultimate necessity of these laws as we are from comprehending the "whys" behind seemingly universal constants like *e*, *i* and *pi*, but we at least glimpse a mechanism whereby these laws, which we must accept simply as givens, are maintained. With the assumption of psi-intercommunication between individuals, this is in principle no more incomprehensible than is any directed activity in a freely intracommunicating group. The essential trick behind the "miracle" of probability has been exposed. In embodying the behavior of the

individual as well as of the group in one system of description, furthermore, we are able to bridge the gap between statistical regularity and individual variation.

There is another aspect to this about which we might say a word. When we follow out the implications of the model of how events come to happen that we have derived from our psychological study of psi-correspondences in humans, we are more or less forced, I believe, to a picture of things essentially similar to that which has been poetically expressed in the great mystic philosophies, those of Lao-Tse, Meister Eckhart, and others. In these systems the growth of a seed, the mind of man and the light from our most distant star are, if I might so put it, reciprocally influential, mutually determinative. This way of expressing things suffers, of course, from our almost ineradicable proneness to view things from the outside, which Lao-Tse and others caution us against when they tell us we must strive to nullify the gap between the "I" and the "not I" in any attempt to "know" what is what. When, however, we stick to the method of description in which we use the correspondence of the psi class in the manner suggested, we can break down to a great extent the distinction between the "I" and the "not I" at the same time as we are forced step by step to connect in one system the doings of the electron, the strivings of man, and the motions of the stars. An anthropocentric bias in our conception of the psi correspondence sooner or later becomes untenable except in terms of ourselves as the map makers.

X

We have tried to minimize the number of separately conceived disconnections with which science has to deal. We as parapsychologists term the disconnection we deal with the correspondence of the psi class. Physicists and biologists no doubt would prefer to call it something else. But whatever it is called, it performs the same service descriptively in linking events which can be ordered meaningfully but not in terms of those congruent pointer readings to which we give the shorthand designation of "force." We have tried to outline a system whereby it might be possible to take care of several unknowns with one basic postulate, much as the Federal Reserve Banking system has been able enormously to expand the monetary credit structure with a single mobile cash reserve. If there be some who prefer to believe that the kiting of correspondences that we do in these maneuvers corresponds to something "inside" or "outside" that may be called causality, I can not personally foresee in this any undesirable consequences. At least such a belief might constitute a "cause," so to speak, serving to unite various factions in an other-

wise loosely knit household, like a decrepit old family servant who no longer gets the stairs swept but whose welfare nevertheless constitutes the one effectual point of liaison between the otherwise mutually disinterested members of the younger generation. One way or the other, however, it no longer appears strictly necessary to divide the universe into two mutually irreducible principles, causality and chance, or causality and synchronicity.[4] These apparent opposites can be satisfactorily brought together and reduced into mutually meaningful terms by the consistent use of our correspondence-making system.

In regard to the question of, "Is there or is there not a power behind the throne," it may indeed be that with so much concrete "causality" *in absentia,* we are led inevitably to a metaphysical God concept. But such a concept need not be a static one, however much of a hard, unfilterable residue may be yet left over after all its transformations. The fact is that the metaphysical God of Whitehead is not quite the same as that of Aristotle: One might say that he has shrunk somewhat (although very little, to tell the truth) in mystery as he has gained in cleverness, much as a magician who has bit by bit revealed his secret methods of legerdemain. In a sense he remains the master magician whether he leads you to believe in the immanent entelechy of his strange automatons or tells you finally that he does it all by cybernetics. But even if one felt one had to rest upon the principle of an ultimate Godhead, it should be possible to do more than proclaim its utter unfathomability and unknowableness. Perhaps the Godhead can be seen and known by means of certain tricks and devices, technical and operational mirrors, if you will, in many modalities now still unperceived or uncomprehended. By progressive devacuumization and definition, by substitution of sharp lineament and positive specification for hazy aura, we can perhaps trade ultimate for penultimate mystery, approach more closely to The Asymptotic Essence. In such an approach, which we might make somewhat in the spirit of Pascal, the psi correspondence would appear at the moment to give us our maximum leverage.

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[4] It seems to me that in any case Jung's distinction between causality and synchronicity is not very helpful. What is generally referred to as causality has fundamentally all the earmarks of what Jung terms synchronicity in that it is found to "consist of," so to speak, nothing but a certain type of meaningfulness; and what Jung terms "synchronistic" occurrences have as much right to be considered "causally" connected, insofar as we can succeed in ordering them at all, as any other events. Whether or not they have a connection "in themselves" may be beyond our power to ascertain; but so long as *we* are able to connect them in ways which we regard as lawful, we have a right to view them, if we so please, as "causally determined." We can do no more in the case of the heavenly bodies.

XI

Before concluding, I would like to say something about those psi correspondences to which the term "precognition" has generally been applied. Most parapsychologists have tended to regard the data of these correspondences as requiring radical alterations in our constructs of time and causality. It is almost axiomatically assumed that on the face of things a complete revolution in our ways of ordering events in relation to these parameters will be required before a satisfactory framework for "precognitive" phenomena will ever be found.

However, parapsychologists have failed fully to explore the logical implications of all the psi data they have, and have given short shrift to hypotheses advanced by investigators who have tried to do so. The most consistent hypothesis of this sort was put forth by Tanagras (16, 17), who attempted to account for all chronologically extraordinary psi correspondences (a term which I prefer to "precognitive phenomena") on the basis of the more readily accepted psi "processes" such as telepathy, clairvoyance and psychokinesis in conjunction with hypotheses involving telepathic influence and control. Malevolent wishes on an unconscious level, corresponding to hexing and the "evil eye" on a conscious one, he believed to play a hidden but none the less greatly determinative role in the affairs of mankind. Tanagras' point in essence was that in the case of all psi correspondences which appear to be "precognitive," the future is not foreseen but created, brought about by means of the agencies he described.

I do not believe that Tanagras' theory, or any other theory that rests exclusively on such simple and ready-to-hand notions as this author invokes, can account successfully for all types of psi correspondences which appear to be chronologically extraordinary, particularly long term correspondences such as those which some of Nostradamus' predictions might conceivably be classified as.[5] However, I do believe that Tanagras' basic hypotheses are in the right direction, and that those of the parapsychologists who feel obliged to postulate novel, hidden "dimensions" (or what not) in regard to time are completely indefensible from every point of view.[6] The

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[5] There is enough ground in the studies and commentaries on Nostradamus to date to warrant a much more searching historical and linguistic investigation of his quatrains than has been done. To write these off as unsuitable for further research is an unjustifiable presumption.

[6] Some of these postulates have been presented in terms which look mathematical, but C. T. K. Chari has shown in a recent study (Time as Minkowski's Fourth Dimension: *Indian Astrological Magazine*, Annual Number, 1952) that there is not a shred of evidence anywhere in mathematics to support the notion that any sense they seem to make in this form is anything but illusory.

necessary assumptions (assumptions about "mind," if we may so put it, rather than about "time") that would be required to correct Tanagras' theory and to bring it into concordance with our data are not only logically conceivable but even positively justifiable in terms of science's present stockpile of correspondences, including those of the psi class.[7] On the other hand, the radical assumptions about time which have been suggested to account for "precognitive" phenomena are irreconcilable on all fronts with all other correspondences known to science. On analysis these assumptions turn out to be magic wands which the looseness of our language permits us to wave about in all seriousness and solemnity.

One of the more significant consequences of bringing the psychoanalytic set of correspondences to bear upon the problem of chronologically extraordinary psi correspondences ("precognition") has been that a number of pieces of a complex puzzle have one by one begun to fall into place, revealing something of the depth and the interrelationships of the all-but-unconquerable resistances to psi which tend to narrow and distort the perspectives not only of science in general but also of the parapsychologists themselves. The working out of this puzzle has in many respects been like the unfolding of a mystery novel, with the resolution of one apparent paradox after another once the long hidden clue has been brought to light (5). The clue in this case seems to be related to one of the most important consequences of the origin somewhere in the dim past of the primordial family institution, with its much prolonged period of maternal dependency, namely, the development on man's part of more and more ineradicable guilt over his destructive tendencies and his consequent need to project further and further from himself both the responsibility for his deeply repressed aggressions and the awareness of his active role in the events that he comes to conceive of as occurring "outside" of and apart from himself. This can be related on the one hand to the peculiarly obscure "now you see it, now you don't" phenomenology of psi and, on the other, to man's ostensible need to project the entire notion of causality step by step from within outward, beginning with the primitive conception of *mana*, progressing through magic and witchcraft to parallel developments

[7] It will be necessary, however, for many parapsychologists first to untether themselves from the completely unwarranted assumption, implicit in much of the current literature, that our best source of information upon which to base inferences regarding the possible range and scope of psi is the laboratory. As a case in point we might cite the scandalously rigid attitudes toward PK which seem to prevail. One might gather that the reporting of observations on the behavior of dice constituted science, while the reporting of observations on the behavior of people — by such disingenuous dabblers as Crookes, Lodge, Barrett and others — constitute merely unreliable contributions to folklore.

in science and religion, and culminating in our day in an epistemological device for getting rid of the basic concept altogether.[8]

I believe that the time may soon be ripe for a counter-revolution in which both the data of psi correspondences and the data of psychoanalysis, particularly those relating to man's hidden aggressions, his defenses against them and the techniques with which he attempts to minimize and deny them, will be brought into a framework for the ordering of experience much more unified and much more self-consistent than now obtains. The problem of the disconnections in science will not then disappear, but it will have a decidedly new look about it.

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[8] See in this connection, Kelsen, Hans: *Society and Nature,* University of Chicago Press, 1943. This author has been able to arrive through anthropological and historical research at a thesis which checks precisely with the inferences to be drawn from a psychoanalytic approach to the problem of the disconnections in science (which can be said to include the problem of chronologically extraordinary psi correspondences). This thesis is that the conventional concept of causality had its origin in primitive fears of retribution for wrong done. Kelsen is able to show, among other things, that the original words for guilt and cause were identical in both the early Indian languages and in Greek, whence they were taken over into juristic forms and later into the writings of the early Greek scientist philosophers.

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Eusapia Palladino in Retrospect

J. FRASER NICOL

Of the many mediums who have professed to demonstrate the physical phenomena of psychical research, the claims of Eusapia Palladino are in some respects the most impressive. Over a period of twenty years she was investigated by a greater number of scientific committees than any other medium in history.

One of the last chapters in her long career has only now been revealed in full by the publication of the transcripts of her American sittings of 1909-1910.[1] As this new account is likely to be the last substantial addition to the enormous literature on the mediumship that we are ever likely to see, it may be worth while to glance back at some of the highlights of this woman's surprising history.

Born of peasant stock in Southern Italy on January 21, 1854, Eusapia was orphaned at an early age and eventually went to make her home in the 1860's with a family who had evinced a lively interest in the new craze for table turning. Eusapia was invited to join their circle, and after ten uneventful minutes, the table took to the air. In the course of further séances it became clear that the source of the manifestations was Eusapia Palladino.

Not until many years had passed, however, was Eusapia subjected to scientific investigation. As a result of an earlier study by Professor Chiaia of Naples, Professor Cesare Lombroso with a group of scientists and scholars held two sittings with Eusapia in 1891 and reported favorably to the medium's claims. Other groups observed the phenomena at Milan and Naples, and Eusapia was soon famous throughout Europe. She travelled widely—St. Petersburg, Rome, Warsaw, Paris—and gave sittings for various international groups. Three highlights of her career best illustrate the enigma of Eusapia's mediumship: her sittings for Charles Richet, the S.P.R. study at Cambridge, and the famous Naples investigation of 1908.

Eusapia's skill in cheating had been detected and studied in the early eighteen-nineties by Torelli Viollier, Richet, and Ochorowicz. Her methods were well known and steps had been taken (e.g., by Richet) to prevent them. It was against this background knowledge that in 1894 Richet, Oliver Lodge, F. W. H. Myers and J. Ochoro-

[1] Hereward Carrington, *The American Séances with Eusapia Palladino.* Pp. viii + 273. Garrett Publications, New York, 1954. $3.75. (Note the spelling of Palladino. In the early literature the name is spelled "Padalino" and later "Paladino." Eusapia spelled her name with two "ll's," as can be seen from the facsimile signature in H. Carrington, *Eusapia Palladino and Her Phenomena,* New York, 1909.)

wicz sat with Eusapia at Richet's home on the Ile Roubaud in the Mediterranean. So impressive were the phenomena observed that Richet later held additional séances near Toulon at which Professor and Mrs. Henry Sidgwick and Baron von Schrenck-Notzing were also present. As a result of these two groups of sittings, the investigators—notably Lodge and, a trifle more cautiously, Mrs. Sidgwick—presented accounts highly favorable to the paranormal hypothesis. Surveying these reports[2] now, however, it does seem clear that while indeed some of the telekinetic effects may have been, and probably were, genuine, there was a proportion that could be very plausibly laid to the credit of the medium's adroitness in hoodwinking her sitters. Throughout her long career, Eusapia's most useful piece of legerdemain consisted in approximating her two (controlled) hands until, with a deft movement, she caused the controllers to share the control of *one* hand while the other slipped free. Her extreme restlessness at the séance table made this trick easy to perform—at any rate with inexperienced sitters.

So far as I am aware, however, Eusapia was never found guilty of fraud prepared in advance of a sitting. On various occasions in her later history she was thoroughly searched and nothing suspicious was found.

The S.P.R. group at Richet's sittings appeared to have been satisfied that, even after making full allowance for Eusapia's tricks, her phenomena warranted further study. Accordingly they invited her to Cambridge, England, the following year, 1895. In the meantime, however, Dr. Richard Hodgson expressed dissatisfaction with the report of the French sittings on the ground that the control of the medium at crucial moments was insufficiently described. Hodgson, then Secretary for the S.P.R. in America, was invited to the Cambridge inquiry.

The Cambridge sittings form one of the most perplexing chapters in the history of mediumship. Eusapia was repeatedly detected in fraud, principally by her hand-substitution trick. Sidgwick wrote that "the investigators unanimously arrived . . . [at the conclusion] that systematic fraud had been used from first to last, and that there was no adequate reason to suppose any supernormal agency whatever."

The report of the Cambridge investigation[3] is most disappointing, for, whereas Hodgson had sharply criticized the French report on the point of inadequate reporting of controls, the Cambridge

[2] *Journal* S.P.R., Vol. 6, 1894, pp. 306-360. For the ensuing controversy between Hodgson, Lodge, Myers, Richet, and Ochorowicz, see *Journal* S.P.R., Vol. 7, 1895, pp. 36-79.

[3] *Journal* S.P.R., Vol. 7, 1895, pp. 131-135 and 148-159.

report is even less informative. Only one of the sittings was published in detail and more than two-thirds of it consists of notes added by some of the sitters at *later* dates. One of the investigators, Oliver Lodge, in a letter subsequently published, made it clear that he did not wish to have his name associated with the "Cambridge investigators." Hodgson and his colleagues had discovered (as Richet pointed out) only what was already well known from the work of previous investigators.

While Sidgwick spoke of Eusapia's "mischievous trade," the Continental researchers were less inclined to moralize. They employed—or professed to employ—methods of control whereby fraud would be excluded. Reports continued favorable (though, to later readers, not always so satisfying as appeared at the time).

For a dozen years Eusapia continued to be the wonder of Europe and the flow of reports endorsing her phenomena encouraged a new and rather younger S.P.R. group to new efforts at Naples in 1908. The investigators were the Hon. Everard Feilding, W. W. Baggally, and (from the American S.P.R.) Hereward Carrington. They were a remarkable group: two of them, Baggally and Carrington, were skilled amateur conjurors; two, Carrington and Feilding, had been investigating alleged physical phenomena for ten years—Baggally for thirty-five years—and they had never experienced a genuine physical phenomenon. Carrington was the author of what is still the standard work on séance-room fraud.[4]

The Cambridge and Naples sittings differ in two important respects. The former took place in almost complete darkness and the medium was given opportunities to cheat. At Naples "many of the phenomena took place in light when her hands and her whole body were in full view."[5] Also the intention of the investigators was to exert a control so rigid that cheating would be impossible. In this last respect Feilding and his colleagues met with much success if one is to judge from the evidence of the shorthand transcript of the sittings.[6]

In these circumstances the table repeatedly rose a foot or more in the air when apparently no person was touching it. In the curtained cabinet behind Eusapia a bell rang, a guitar was strummed, and a table carrying various objects was moved about. The famous

[4] *The Physical Phenomena of Spiritualism.* First Edition, 1907; 3rd Edition, 1920. In the first edition Carrington adopted an "extremely skeptical" view of Eusapia, an opinion which he retracted after his Naples experience in the following year.

[5] W. W. Baggally, *Journal* S.P.R., Vol. 14, 1910, p. 209.

[6] E. Feilding, W. W. Baggally, & H. Carrington, "Report on a Series of Sittings with Eusapia Palladino," *Proc.* S.P.R., Vol. 23, 1909, pp. 309-569.

breeze from a scar on Eusapia's forehead was detected and a hand was repeatedly seen carrying objects out of the cabinet while her normal hands were held by the investigators.

One of the most interesting things about this report is the persistent skepticism of all three researchers. On the morning following a sitting, they would re-read their notes and again lapse into doubt on the ground that since these events were in conflict with the known laws of nature they could not have happened. Nevertheless, having excluded all explanations known to them (including collective hallucination), they delivered a unanimous report affirming the paranormal origin of the phenomena.

In those Naples séances Eusapia reached the zenith of her career. After that came the descent, the first signs of which were in the American sittings now reported.

She arrived in New York on November 10, 1909, and sensational accounts appeared in the newspapers next morning of demonstrations of her powers given by Eusapia on shipboard. The first séance in the United States was given for the benefit of newspaper reporters, Dr. Carrington believing that this favor would keep the press quiet for the ensuing months of Eusapia's visit. This hope was falsified by events. And the story of the American venture is mixed up with the mishaps that so often attend newspaper publicity in psychical research. When journalists were not at hand, some sitters could be depended upon to describe their supposed experiences to the papers and magazines. Professor Hugo Münsterberg wrote a sensational article for *Collier's*, Professor Joseph Jastrow attended one séance and wrote two articles about it. A professor of philosophy retailed his findings in the *New York Times*, and so on.

Such was one burden. Another and perhaps more important one lay in the inexperience of so many of the sitters. So far as one can see, almost the only investigator competent to deal with the problems at issue and to forestall the risks of fraud was Dr. Carrington himself. But, through the seven months of the sittings, though Carrington was usually present in the séance room at Lincoln Square Arcade, New York, he left the control of the medium largely in the hands of inexperienced visitors. No doubt his course may have seemed desirable at the time, since he mentions that his relations with the sitters were "to some extent financial as well as scientific" (p. 98), and elsewhere he is described as the medium's "manager" (p. 172). Whatever the reasons, the absence of skilled controllers impairs the value of the report. Carrington did, however, send a wise letter to all prospective sitters describing "Eusapia's chief method of trickery" and advising as to the best methods of preventing it.

In spite of the clear instructions, one has frequent doubts as to the adequacy of the control. On the rare occasions on which Carrington himself came forward to take a direct hand in the control, the thoroughness of the detail in the report increases remarkably. For example, on one occasion (January 8, 1910) there is a succession of supposedly striking phenomena occurring over a period of ten minutes, but not a word from the controllers, Professor Busch and Professor D. S. Miller, until it is all over, when they claim that "the control has been continuous." By contrast with these blanket assurances, there is the sitting of November 14, 1909, in which Carrington plainly kept a firm grip on the scientific requirements. He gives an almost minute-by-minute account of the medium's whereabouts and actions, and admonishes other sitters to beware of tricks. At one point the left-hand curtain of the cabinet behind the medium is blown out over the table, and Carrington says, "That is the sort of thing which happens when she has released one hand." We have Carrington giving minutiae of his control, whereas with regard to other controllers, we have to be satisfied too often with vague and belated assurances.

Still, taken all in all, and keeping in mind the authenticity of the accounts presented by the S.P.R. group at Naples, and by the Psychological Institute in Paris, it is readily apparent that many of the phenomena asserted to have occurred in America were of the same form. They may be summarized thus:

1. Telekinesis. The séance table (weight 12 lbs.), was observed in fairly good red light to rise as much as two feet in the air on a number of occasions without visible means of support. Eusapia always sat outside the curtained cabinet; yet within it, a small table and musical instruments were knocked about and sometimes projected into the room.

As a subdivision of the telekinetic phenomena, we may note the production of sounds from the musical instruments. Thus, while Eusapia would flick her fingers back and forth in the air, a mandolin in the cabinet behind her would sound in unison with her movements. This had apparently been shown at earlier times not to be due to any thread or other object connecting the medium with the instrument. If true, it suggests that Eusapia could exercise *conscious* control over the queer events occurring in her presence. Indeed when she felt that some phenomenon was about to appear, she would often cry out for closer control.

2. The appearance and disappearance of objects of unknown origin. These were of irregular structure, sometimes hand-like in shape, and they usually emerged from between the curtains. These

are the most inexplicable, and indeed the most suspect manifestations, and yet when the medium was searched, nothing was ever found to account for them.

3. The "breeze" from a scar on the right side of Eusapia's forehead. This had been noticed at séances and in clear light at other times by many investigators. It could not apparently be explained by any novel use of the medium's breathing apparatus, and its source remains to this day one of the quainter mysteries of Palladino.

In January, 1910, the medium's services were transferred to a group of scientists and philosophers at Columbia University. Four sittings were held in eight days. Twelve persons were present at these séances. Carrington was excluded, and the status of the eminent men who took part can be described without offense as that of earnest apprentices. There must be few séance records so disappointing as those of the four Columbia sittings. Time and again one is led to suppose that the control was "perfect" (a favorite Columbianism) and yet when we want to know the whereabouts of Eusapia's hands and feet and of the scientists' own hands at the exact moments of the phenomena, the sitters forget to tell us.

The truth of the matter seems to be that they were inadequate to their task, and that in the first three sittings they were undergoing a hasty education in the complexities of the séance room. At the fourth and final sitting they determined to do better, and imposed what they apparently believed were methods that would make "trickery absolutely impossible." In a statement published in *Science*, May, 1910, they declared that under those conditions "none of the so-called evidential phenomena took place." So far as I am aware, no full account of their sittings was ever published by the Columbia group, but the stenographic report is now before us, and here their contemporary remarks to the stenographer are plainly at variance with their comprehensive assertion in *Science*.

With regard to the controls, there are several examples of sitters disputing each other's assurances. With regard to the phenomena, no doubt the medium was being put on her mettle, and the demonstrations were certainly of a reduced variety. Nevertheless there were innumerable curtain movements, some table tilting, "touches" were felt by sitters—all under supposedly good conditions of control.

It is perhaps not surprising that the work of the Columbia committee was so unsatisfactory, for Carrington tells us (p. 232) that "not a single member of this committee had taken the trouble to inform himself on the history of the case," nor had they read any of the reports written by scientists more distinguished in psychical research than themselves.

Accounts of thirty-one séances in all are given in the book, including the shorthand transcripts of twenty-eight of them. What conclusions can be drawn? Because of the inexperience of the sitters, the records are extensively imperfect, the controls are often inadequate, and there seems no reason to doubt that fraud was frequently perpetrated. Examples, however, can be found—the first and tenth séances are instances—where some of the manifestations cannot easily be fitted to a "normal" hypothesis. If they stood alone, the American séances could satisfy no critical-minded reader, but taken in conjunction with earlier and better investigations, it may be supposed that some at least of the positive but restrained views expressed by Dr. Carrington can be supported by fairly good evidence presented in the book.

Eusapia spent seven months in the United States, and when she sailed for Naples on June 18, 1910, her career was virtually at an end. She gave a few sittings for an S.P.R. group in November-December of the same year, but the conditions were lax, and the results unsatisfactory.

Over a period of years her health seems to have undergone a slow decline, and she died on May 16, 1918.

As to the *nature* of the telekinetic force which she ostensibly demonstrated, her contemporaries were hardly any wiser at the end of her career than at its beginning. Instrumental and psychophysiological methods, occasionally employed, yielded inconclusive results for the most part. The value of Eusapia Palladino to psychical research lay elsewhere. Her readiness to be investigated by competent observers permitted the development of rigorous methods of control and of exact recording of facts at the moment of their occurrence to a degree wholly unknown before her time. Experimenters of the next generation profited from the impressive standards created by Feilding, Baggally, Carrington, and others. Let us hope that their example will not be lost sight of in the future.

An Examination of the Humphrey-Nicol Experiments on the Feeling of Success in ESP

ROBERT H. THOULESS

Dr. Humphrey and Mr. Nicol have made a praiseworthy attack on a problem which has been incidentally considered by a number of other parapsychologists, as to whether subjects who are doing ESP experiments can to any extent know whether they are guessing correctly or not.[1] The general opinion has been that even very good ESP subjects are unable to make correct reports as to which of their guesses are correct in the experimental situation although spontaneous cases of ESP suggest that those experiencing them often have also a conviction that these do represent reality. It is obvious that failure in the past to demonstrate any knowing by the subject of when he is guessing correctly is insufficient ground for conviction that nothing of the sort will be discovered in the future and the question remains open as a research problem.

The authors of this paper present evidence which they consider shows that their subjects were able to some extent to make this discrimination between right and wrong guesses. If this result is confirmed, it will be an important finding. Unfortunately most of the evidence presented cannot be regarded as proof of this hypothesis since the results obtained could be explained in another way. Some of the statistical estimation also appears to be faulty. There remains at least one strong indication (in Table 12) that the subjects were succeeding to a small extent in the task of discriminating between right and wrong answers.

The subjects were asked to call "check" when they felt sure that the guess they had made was correct, and the question the authors asked was whether the percentage of checked responses was greater amongst the hits than amongst the misses.

The main part of the evidence is presented in Table 6 in which the totals of checked and unchecked hits and misses are shown for the series of experiments with 34 subjects under "unknown" conditions, i.e., when the subject was not told of his success or failure till after the completion of the run. The results are as follows:

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[1] Betty M. Humphrey and J. Fraser Nicol, "The Feeling of Success in ESP," JOURNAL A.S.P.R., Vol. XLIX, 1955, pp. 3-37.

	Hits	Misses	Total
Checked	407	1431	1838
Not Checked	905	4057	4962
Total	1312	5488	6800
Per cent Checked:	31.0%	26.1%	

$\chi^2 = 13.13$
$P = .0003$

This is only part of the story since there was also a series of experiments under "known" conditions, in which the subject was informed of success or failure immediately after guessing, in which there was no significant tendency to check a higher proportion of hits than misses. Even considered, however, as the best of two series of experiments, the "unknown" results are impressively significant and, at first sight, appear to support the authors' conclusion that their subjects were able to distinguish to some extent between success and failure.

This conclusion is, however, fallacious since the results might be due to a very different cause. If those subjects who showed high scoring also happened to call "check" more often, the above result might follow even though any one subject were no more likely to call "check" for his hits than for his misses. It is obvious that such a tendency for higher scoring subjects to call "check" more frequently both for hits and for misses would not imply any paranormal capacity to know that they were scoring highly; it could be determined by their previous experience of having high scores. In fact an individual who habitually found that he had high scores might very well develop a higher degree of confidence which led him to say "check" more often.

This possibility may be illustrated by a fictitious numerical example. Let us suppose that two subjects A and B have each made 100 guesses with the following results:

A.	Hits	Misses	Total
Checked	4	16	20
Not Checked:	16	64	80
Total	20	80	100
Per cent Checked:	20%	20%	

B.	Hits	Misses	Total
Checked	16	24	40
Not Checked	24	36	60
Total	40	60	100
Per cent Checked:	40%	40%	

It is obvious that in neither case is there any evidence of the subject knowing when he is right. Both subjects are as likely to check misses as hits.

But when these two sets are added together, the following result is obtained:

A + B	Hits	Misses	Total
Checked	20	40	60
Not Checked	40	100	140
Total	60	140	200
Per cent Checked:	33.3%	28.6%	

The mixed scores look like the results of the Humphrey-Nicol experiments shown in the earlier table, and would seem to suggest that subjects knew to some extent when they were guessing right. This appearance is, however, misleading since neither A nor B showed any signs of knowing when they guessed right. It is due

merely to the fact that B both guessed right more often than A and also checked more often than did A.

All the evidence of this kind presented by the authors must therefore be dismissed as fallacious. More cogent evidence could be obtained either by treating the results of each subject as a unit and seeing how many subjects showed a higher proportion of checks on hits than on misses or by treating each run as a unit and performing a parallel calculation. The authors have presented evidence of the first kind. It is, however, not statistically significant.

The evidence is given at the bottom of page 23. In the "unknown" experiments, 23 subjects gave a greater number of checks to hits than to misses while 11 gave a greater number of checks to misses than to hits. The chance expectation is, of course, 17 in each group. For this discrepancy from expectation chi^2 (with Yates's correction for discontinuity) is 3.56. This gives $P = .06$ which would be only marginally significant even if this were the only set of experiments to be considered. But there was also the "known" set of experiments which gave no indication of the effect. If we consider that the present experiment is the more striking result of two sets of experiments, we must ask what is the probability that at least one of two sets of experiments will show a deviation from expectation equal to or greater than this. The required probability is .12 which is clearly beyond the margin for significance.

The authors give what appears to be a considerable over-estimate of significance when they say (p. 23) "the probability here is .012." It is to be supposed that the quantity referred to is P/2, the probability of a deviation of at least this size in the observed direction occurring in a chance series whereas the chi^2 method gives the probability of a deviation in either direction, but even for P/2 this estimate does not seem to be correct. It is true that the chi^2 method gives only a close approximation to P, and the exact criterion in this case is, as the authors point out, obtained by summation of the binomial expansion of $(\frac{1}{2} + \frac{1}{2})^{34}$. They say, however, in a footnote that summation of the binomial is laborious, so they used the approximation given by Pearson's incomplete beta distribution. They perhaps exaggerate the labor of summing the binomial series. I have done so and find that P/2 is .03, which gives the same value of P as that given by the chi^2 method. The choice of the incomplete beta distribution was perhaps unfortunate since the effect of using a continuous function for a discontinuous variate is to overestimate the significance, although I should not have expected the overestimation to be as great as that represented by the difference between .03 and .012.

In view of the fact that, when correctly estimated, the value of P for the results as treated on p. 23 does not reach significance, this part of the work also cannot be regarded as evidence for the reality of any capacity for discriminating between right and wrong responses although a significant result here would have been valid evidence. There is, however, better evidence in favor of the authors' hypothesis in an interesting table on p. 24 which shows the number of checks given to right and wrong answers in runs with different scores.

The first point of interest in this table is that one may hope to obtain from it some indication as to whether the main results could have been due to high scoring subjects giving the greatest number of checks both to hits and misses. The indications are not altogether clear but there is some suggestion that high scoring runs contain a large number of checks. In the runs with a score of 9, for example, there are 9.33 checks per run which is the largest total number, the next highest being 7.62. In these runs too there is the largest proportion of checked misses, 35.4%. At the other extreme, the runs with a score of one show the lowest number of checks per run: 6.36. There is not, however, any continuous gradation between these extremes; the runs with a score of 2 give the second largest average number of checks per run. The general appearance of the table is not inconsistent with the hypothesis that the main results could have been due to high scoring subjects giving the most checks.

On the other hand this table yields one piece of evidence which cannot be explained by this hypothesis. As the authors point out, all collections of runs with the same score show a higher proportion of checks on hits than on misses. This result is significant ($P = .004$) and even when we take into account that no such effect is reported for the other "unknown" series, the result remains comfortably significant (P less than .01).

While this may be regarded as an encouraging indication that there may be some power of discrimination between right and wrong answers in this experimental series, it must, of course, be treated with some reserve as evidence. It was not the way in which it was intended that the experimental results should be tested and the appearance of significance may be illusory since if a sufficient number of unexpected oddities of the data are examined some will be found to have low values of P even if there is no real cause producing a departure from randomness. It cannot be regarded as proof but as a pointer for further research. No claim for any value as proof of this peculiarity of the figures is, of course, made by the authors who are careful not to base conclusions on unanticipated characteristics of their data.

SUMMARY.

The main argument on which the authors base their conclusions is fallacious since their results could be explained by the possibility that high scoring subjects made the most checks without any assumption that subjects could discriminate between right and wrong calls.

The argument based on the number of subjects calling "check" more often for right answers than wrong would be valid if the observed deviation from expectation were significant. It is, however, wrongly claimed by the authors to be significant.

The observation that all run lengths show a higher proportion of checks for right answers than for wrong ones is significant but its value as evidence is limited by the fact that this was not the way in which it was intended that the data should be examined.

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An Answer to the "Examination"

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If we understand it correctly, the argument in the above "Examination" runs as follows:

1. The statistics may involve a fallacy.
2. Therefore they do involve a fallacy.

Concerning 1) we are given the imaginary and extreme examples A, B, and their Total. Concerning 2) we have the statement: "All the evidence of this kind presented by the authors must therefore be dismissed as fallacious."

It will be shown that what is weak in logic is empirically baseless.

Before ever the "checking" experiments began, close study was given to the statistical problems of estimation. At the conclusion of the experiments further work was done, both theoretical and practical. We were fully aware of the controversies that have enlivened the history of contingency table methods in the last half century, and we are not so unacquainted with the terrain as to slip into a pitfall that was exposed by Udny Yule more than forty years ago.

Our answers to the above criticisms will be taken up under three headings:

1) Relation of checking *frequency* to run scores.
2) Relation of checking *frequency* to subjects' total scores.
3) Relation of checking *success* to run score levels.

Relation of Checking Frequency to Run Scores

The statistical "fallacy" is conjectured to have arisen by high PNC (paranormal cognition) scores receiving *many* checks while low scores received *few*. Combine these two categories and the checking "fallacy" appears.

Let us then compare the *number* of checks given on high run scores with those given on low run scores. The "Examination" presents extreme examples. In our Unknown data (the section in which the checking effect appeared), the most extreme cases we can find are one run with 11 hits and one run with zero hits. On the former there were no checks; on the latter, six. Comparing these, we have:

	PNC SCORE		
	0	11	Total
Checked Trials	6	0	6
Unchecked Trials	19	25	44
Total	25	25	50

By direct computation the probability associated with this table is .022. This is significant, but the distribution of the checks is directly *opposite* to that predicted by the "fallacy" theory.

The number of trials and checks in the above case is small; and it is incumbent upon us to examine other extreme comparisons. Thus we may compare other pairs of extreme scores. The next step then is to sum the number of checks on the above score of 11 with the checks made on run scores of 9 (there were no run scores of 10 in our data). Those are to be compared with the number of checks given on runs with scores of 0 and 1. This test gives the result:

$$\chi^2 = 2.45 \qquad P = .12$$

This also does not support the "theory." There is no significant difference between the number of checks given on these high and low scoring runs.

Let us go further by comparing the number of checks on run scores of 11, 9, and 8 with those on run scores of 0, 1, and 2. Result:

$$\chi^2 = 0.386 \qquad P = .53$$

This also is a random effect. Comparing now the number of checks given on run scores of 11, 9, 8, and 7 with those of 0, 1, 2, and 3 gives

$$\chi^2 = 0.588 \qquad P = .44$$

The difference is again very close to chance expectation.

To complete the examination, we compare the number of checks on *all* run scores above chance ("high") with the number of checks on *all* the run scores below chance ("low"): that is, checks on scores of 11, 9, 8, 7, 6 compared with those on run scores of 0, 1, 2, 3, 4, Result:

$$\chi^2 = 0.0276 \qquad P = .87$$

This chance result is conclusive.

In brief, the difference between the number of checks given on low scoring runs and the number given on high scoring runs is a triviality. (Per run: High, 6.81; Low, 6.86.) Thus so far as *runs* are concerned there is nothing in the theory at all.

In our papers we have presented a larger amount of numerical data than was strictly necessary. The justification was that if doubts arose in the thoughts of inquirers, they could compute other statistical results from the printed data. In the paper under discussion all the data necessary for the above calculations were quickly available from Table 12 and the facing page.

Relation of Checking Frequency to Subjects' Scores

The issue relating to the number of checks on high and low scoring *runs* having been disposed of, we now turn to the classification of high and low scoring *subjects*. Each person in the experiment did eight runs (Unknown). The question raised is: Were those who, in their eight runs, scored high more prodigal with their checks than those who scored low? According to the "fallacy" surmise, the answer is Yes. The facts are as follows:

In the context of the 34 subjects none of the PNC scores was particularly high or low; around an expected mean of 40, they varied from 31 to 52. Ten subjects scored above the mean, twenty below the mean, and four scored at the chance level. They were irregularly dispersed and do not lend themselves to the tidy "converging" comparisons which we have applied to the runs. However, for what it may be worth, we may compare the quantity of checks given by those whose PNC score was six or more points above chance with the number of checks given by subjects whose PNC scores were six or more points below chance. The result is:

$$\chi^2 = 0.0091 \qquad P = .93$$

This disagrees with the "theory." There is relatively no difference in the number of checks given by the highest and lowest scoring subjects. A more extensive test is to compare the checks of *all* sub-

jects whose PNC score was above the expected mean with the checks given by *all* subjects who scored below the mean. This gives:

$$\chi^2 = 2.06 \qquad P = .15$$

Thus there is no overloading of checks by high scorers or parsimony of checks on the part of low scorers.

Finally to complete this section, we considered the sixteen PNC score categories observed in our data, these ranging as stated from 31 hits to 52 hits (there were several blanks), and determined whether the number of checks varied between these classes. Analysis of variance provided:

Source of Variance	Degrees of Freedom	Sum of Squares	Mean Square	F
Between PNC Scores	15	2295.463	153.031	1.60
Within PNC Scores	18	1724.420	95.801	
		P = .17		

The probability of the computed value of F is .17, which being insignificant, leads to the conclusion that within the range of random variation, the checks are impartially dispersed through the subjects' PNC scores 31 to 52. The notion that high scoring subjects gave a disproportionately large number of checks and the low scorers disproportionately few is empty. Had there been a positive correlation between number of checks and size of PNC score, a different type of statistics would have been called for.

Relation of Checking Success to Run Scores

Now, having disposed of the problem with respect to the simple *amount* of checking on run scores and on subjects' total scores, we turn to a more interesting aspect of the whole matter. This will serve a double purpose by 1) giving further evidence in refutation of the "fallacy" speculation, and 2) giving evidence of the extensive nature of the *checking effect* in action.

We consider the actual success or failure of checks at all run score levels from 1 to 9. This is done by the matching method (see footnote 2, page 7 of the original report) by which it is easy to derive mean chance expectation, deviations from this, and the variance for each score level. The following table gives the necessary information:

Run Score	1	2	3	4	5	6	7	8	9	Total
No. Runs	11	21	38	49	53	45	27	20	6	270
Deviation from Expectation	+4.40	+6.40	+3.68	+11.92	+11.60	+37.36	+22.32	+1.92	+3.68	+103.28

It will be noted that the checking effect is positive at every scoring level. The probability that nine results should all be in the same direction is .0039. Chi square derived from summing deviations and variances is 13.34 ($P = .00026$) which is in close agreement with that found from the 2 x 2 Table 6 in the report ($\chi^2 = 13.13$).[1]

Since it was previously found that the *number of checks* approximates to equality for the nine scoring levels, it is appropriate to inquire whether the "strength" of the *checking effect* is greater at some scoring levels than at others. The answer to this question is obtained by first of all 1) computing χ^2 for each of the nine score levels and summing them, and then 2) computing χ^2 for the total. For the former, $\chi^2 = 19.71$ with 9 degrees of freedom. For the latter, $\chi^2 = 13.34$, as previously found. Subtracting the latter from the former, we get $\chi^2 = 6.37$. There are 8 degrees of freedom and $P = .61$ showing that, so far as these data go, there is no reason to suppose that checking is more effective at one scoring level than at another; and that the claim in the "Examination" that "All the evidence of this kind presented by the authors must therefore be dismissed as fallacious" is baseless.

A curious contrast to the above results is obtained when we apply the same methods to Examples A and B of the "Examination." The results are:

Example	Run Score Average	Checks: Deviation from Expectation	χ^2	Degrees of Freedom
A	5	0	0	1
B	10	0	0	1
Sum		0	0	2

The χ^2 for the two tables combined in one (A+B, not given in the above table) is small (0.45), but it precludes taking the analysis any further, since subtraction of this χ^2 from the χ^2 of zero (at foot of the table) would give a *negative* χ^2, which is absurd since χ^2 is essentially positive. As the table stands, the results are shown to be consistent only in their nullity, and being frankly acknowledged "fictitious" seem to have no relevance to the experimental realities.

The chief criticism being disposed of, the others being subsidiary to the main statistical result are now of less significance. With regard

[1] Table 6 in the report contains two more runs than the above analysis because in the latter the one run with zero PNC score and the one run with zero checks had to be omitted. Such data are not susceptible to individual computation since they involve multiplication by zero.

to the Incomplete Beta-Function, there is an error in our paper and a misconception in the "Examination." A word or two about the I.B.F. table (for short). This is no ordinary table, for it runs to 430 pages and some one hundred thousand entries arrayed in about two thousand five hundred columns. By malign (but fortunately not fatal) influence, our collective finger appears to have waywardly side-slipped among the mass of figures and provided P = .012, whereas the proper entry gives P = .0288. We are glad to make this correction and express our thanks.

When, however, the "Examination" notes as "unfortunate" the choice of the I.B.F. as a source of binomial probabilities, it is sufficient to mention that that view is in sharp conflict with the writings of Professors Karl Pearson, E. S. Pearson, M. G. Kendall, and Sir Ronald Fisher.

The I.B.F. gives probabilities to seven decimals. We have summed the binomial distribution and here give the two results for comparison:

	Probability
Binomial Distribution	.0288063
Incomplete B-Distribution	.0288063

The probabilities are identical and do not support the opinion that "the effect" of using the I.B.F. "is to overestimate the significance." We believe this fact ought to be stressed for it would be regrettable if new and inexperienced computers coming into psychical research were to be unnecessarily discouraged from using a most valuable and time-saving tool.

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This organization later became a branch of the English Society under the very able guidance of Dr. Hodgson until his death in 1905. The American Society for Psychical Research was then re-established with James H. Hyslop, Ph.D., formerly Professor of Logic and Ethics in Columbia University, as its Secretary and Director.

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